

Why Ampère did not discover electromagnetic induction

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In 1832, after Michael Faraday had announced his discovery of electromagnetic induction, Andre-Marie Ampère claimed that he had actually discovered the induction of one current by another in 1822. In fact, he had, but did not really publish the fact at that time. This article explores the reasons for Ampère's failure to lay claim to a discovery that would have guaranteed him scientific immortality.

On 4 September 1820, Francois Arago announced to a skeptical audience at the Paris Academy of Sciences that Hans Christian Oersted had discovered that an electric current in a wire would cause a compass needle to deflect if the needle were placed parallel to the wire. On 11 September 1820, Arago illustrated the effect before the Academy, thus removing the doubts of its members who had long been suspicious of "great" discoveries by German nature philosophers. Andre-Marie Ampère attended both these sessions, and immediately realized that Oersted had not carried his investigations far enough. He, therefore, immediately set out to, as he put it, "complete the work of the Danish professor."¹ For Ampère, this meant taking into account the earth's magnetic field, which he did by inventing an astatic needle, and then completely mapping the effects of the current on the needle. It was Ampère, not Oersted, who first reported that the needle set at 90° to the current.

In the course of mapping, Ampère made his fundamental discovery, which was to be the foundation of all his later work. Contrary to the theory then accepted, which argued that the voltaic pile acted like a battery of Leyden jars, first charging and then discharging through the conjunctive wire, Ampère discovered that the same current passed through the pile as through the wire. Such a current was easily detected by the compass needle, and he realized that he had a new electrical instrument which he promptly named a galvanometer. With the galvanometer, it was now possible to detect electric currents without interrupting the circuit to use electrochemical decomposition or the electric shock across one's tongue or eye, as had previously been the case. And then an idea struck Ampère! If a voltaic pile were made in a circle, there would be no evidence of a current *except* the magnetic effect, and this magnetic effect was exactly similar to the magnetic field of the Earth. Thus was born Ampère's basic idea for all his further researches,

that magnetism was nothing but electricity in motion in circles. On 25 September 1820, he showed that a wire coiled into a spiral acted just like a magnet, and that two current-carrying spirals acted just like two permanent magnets. A few weeks later he discovered that two rectilinear current-carrying wires attracted and repelled one another according to the directions of their currents and, with this knowledge, he was able to construct current-carrying helices that exactly (he thought) mimicked the behavior of permanent, bar magnets.² For the rest of his life, Ampère was to insist that *all* magnetic effects could be duplicated by electric currents in wires of different configurations.

Ampère's basic insight, of course, raised an immediate problem. If magnetism were merely the result of circular electric currents, where were these currents in permanent magnets? The most obvious answer was to suggest that the currents were coaxial, and created by the conjunction of the molecules of iron and steel of which the permanent magnets were composed, much as ordinary voltaic currents were created by the contact of two dissimilar metals. Ampère's good friend Augustin Fresnel rather excitedly published a short note in the *Annales de Chimie* announcing the discovery of such currents, but almost immediately had to retract it in the face of further investigations that showed the effects were purely chemical. At the same time, Fresnel suggested to Ampère that he put the currents *around* the molecules of the magnet, since the alignment of such molecular currents would have exactly the same effect as currents coaxial to the magnet itself. Ampère leaned towards this approach, but sought some kind of experimental test that would give it empirical probability. He was not to find such a test until July 1821.

Oersted's discovery and Ampère's brilliant, but sometimes confused, articles extending Oersted's work created a flurry of research throughout Europe with weird and often bizarre experiments reported in all the learned journals, together with even weirder and more bizarre theories to explain them. It was this situation that stimulated Richard Phillips, editor of the *Annals of Philosophy* in London to ask his friend, Michael Faraday, to write a short history of electromagnetism in which scientific wheat could be separated from illusionary chaff. Faraday, who had shown only a passing interest in electricity and magnetism to this point and whose growing reputation was as an analytical chemist, agreed to do the job. The results were to change the history of science.

In September 1821, Faraday published the first installment of his history, which carefully examined the major experimental work in the field. Some six months later, he published the conclusion, which analyzed and criticized the theories that had sprung up. Ampère apparently never saw this work, nor did he know that Faraday had published it since it was the only work Faraday ever did to which he did not sign his name. Its importance was to focus Faraday's attention on electromagnetic phenomena and to start him on his career as the architect of field theory.

It was in the course of repeating the various electromagnetic experiments that Faraday realized that a magnetic pole, if isolated in some way, would revolve around a current-carrying wire, and vice versa. Almost immediately (i.e., in September 1821), Faraday devised his famous electromagnetic rotations apparatus which illustrated both these peculiar effects. Faraday also took the occasion to criticize severely Ampère's theory and to use his "discovery" of the circular line of force to cast doubts upon Amp-

ère's ideas. These doubts were supported by simple but extremely clever experiments.³

To Faraday, his most important discovery was that the "line of force" surrounding a current-carrying wire was circular; this is, in fact, the birth of the concept of the line of force in electromagnetism. More specifically, Faraday used the line of force to challenge Ampère's claim that the forces between electrical elements were central forces, and insisted upon their circularity. With these lines, Faraday could explain all the electrodynamic effects that Ampère had discovered and, indeed, was able to go beyond Ampère with his construction of his rotation apparatus. This was a real embarrassment to Ampère, for Faraday had discovered an effect which had escaped his attention, although he was quick to point out that the rotations could easily be deduced from his theory. Nevertheless, the combination of the new discovery and Faraday's criticism could not go unanswered, for both seemed to strike at the very foundations of his new science. Ampère responded by footnotes appended to the French translation of Faraday's article in the spring of 1822. We shall here look at both the article and Ampère's response.

Besides his insistence on the circularity of the electromagnetic force, Faraday also devised some experiments which he felt were serious obstacles to the acceptance of Ampère's theory. The first was a simple mapping. If, as Ampère insisted, magnetic effects were nothing but the results of electrical currents in circular motion, then a current-carrying helix and a bar magnet should be *exactly* alike. Faraday insisted that they were not. If the lines of force of both are carefully mapped, the "poles" of the permanent magnet are discovered to be some distance in from the ends of the magnet, whereas the "poles" of the helix are exactly at the ends.⁴ Even more cutting was another set of experiments. To illustrate vividly his contention that electromagnetic attractions and repulsions were illusory results of the tendency of magnetic poles to rotate around the circular line of force generated by electric currents, Faraday devised a lovely demonstration. He took a glass tube and wound an insulated helix around it, and then half submerged the tube in a pan of water so that the plane of the water bisected the tube. He then took a long magnetized needle and floated it on the water. When the current in the helix was turned on, one end of the helix became a north pole and the other a south pole. The needle was now placed so that its south pole, say, faced the helix's north pole. If the forces between these two poles were simple attractions, the needle should float up to the north pole of the helix, and then stop. It does not. It continues on *through* the glass tube until south in the needle is under south in the helix. If that south pole were free to move, i.e., a monopole, Faraday argued, it would continue to move along the line of force surrounding the helix, with no attractions or repulsions involved. In and of itself, this experiment merely intensified Faraday's rejection of Ampère's theory of central forces. But it also provided him with a way of again testing Ampère's contention that *all* magnetic phenomena could be reproduced by electric currents in proper configurations.

What Faraday now did was to take a piece of sheet steel and roll it into a hollow cylinder welded together along the seam. This cylinder was then magnetized and Faraday insisted that it now represented the magnetic equivalent of the helix around the glass tube. This cylinder was then placed in the water, exactly as the glass tube had been, and

the magnetic needle allowed to float freely towards it. This time, the south pole of the needle *stopped* when it came up to the north pole of the cylinder. The two effects could hardly have been more different, and Faraday challenged Ampère to explain the two anomalies of the locations of the poles and the opposite behavior of the helix and the hollow steel magnet.

It was with some satisfaction that Ampère met Faraday's challenge. He first disposed of the "circular" line of force by arguing that this *must* be a resultant force, not a "primitive" one for no other such forces were known in nature. The argument, it should be noted, did not impress Faraday, although it seems to have been quite acceptable in the French physics community, which recognized only central forces.⁵ The location of the poles in a permanent magnet was capable of explanation by means of a most important experiment performed by Ampère in July 1821. It was this experiment that was, with slight modification, to lead to Ampère's discovery of induction in 1822, so it is worth considering closely.

In January 1821, Ampère gave a report to the Academy of Sciences in Paris in which he briefly considered the location of the electrical currents required to produce magnetic effects in permanent magnets. There were, he noted, only two possibilities: Either the currents were "macrocurrents" circulating around the axis of the magnet, or they were "microcurrents" circulating around the particles of the magnet, i.e., molecular. Since the macrocurrents were completely undetectable experimentally, Ampère came down tentatively on the side of microcurrents although these, too, could not be detected. The experiment he did in July was intended to decide between the two types of currents.

A coil of many turns of insulated wire was fixed on a stand, with leads to be connected to a voltaic pile. A thin strip of copper was formed into a circle and suspended by a thin wire, passing through the coil, so that it hung just inside the coil. The coil was then connected to the pile, and a bar magnet brought up to the copper strip. Ampère's thinking here seems simple. He knew that the electric current of a helix would induce magnetism in an iron bar with-

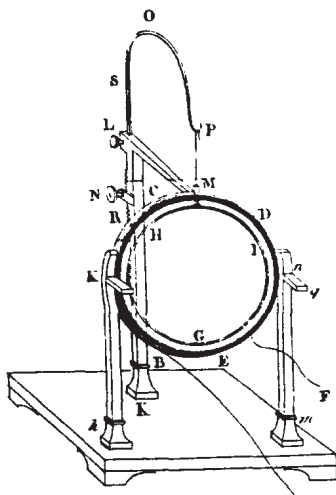


Fig. 1. Ampère's apparatus for his experiments in 1821 and 1822. HIG is the copper sheet suspended from P. DRE is the coil of wire connected to the battery by the wire F and the one leaving the coil at B.



Fig. 2. Ampère's illustration of the mutual effects of molecular currents distorting the magnetic force so that the poles of the magnet do not occur at the very ends.

in the helix. Since he also "knew" that magnetism must be the effect of electric currents, this meant that, somehow, the currents in the helix either induced currents in the iron, or aligned previously existing currents around the molecules. His experiment was to test the induction. He could observe no magnetic effect when the magnet was brought close to the copper strip and the current was on in the coil, from which he concluded that no coaxial, macrocurrents were induced. It then followed that the currents in magnets *must* be around the molecules of the magnet. (See Fig. 1.)

Molecular currents now permitted him to explain the location of the poles in permanent magnets. Given the mutual forces that must exist between his electrodynamic molecules, these molecules could not line up in neat, parallel rows. Instead, toward the ends of the magnet, they must be pulled out of line so that the poles now would appear to be some distance in from the end. (See Fig. 2.) Note that such an explanation was not available in the case of coaxial currents where no such distortion would appear. Ampère was now locked into the explanation of magnetism as molecular electrical currents.

That position was further fortified by his escape from the trap Faraday had set for him with the hollow steel magnet. Faraday's experiment contained a nice loophole. The helix and the hollow steel magnet could be considered as exactly analogous *only* if the currents in the magnet were considered to be coaxial. But, as Ampère was quick to point out, the situation was quite different if the currents were molecular. Ampère's words here are worth citing.

When one supposes that the currents in a magnet are around its axis, then the analogy with the hollow cylinder and the helix ought to be exact; but not if one admits, as M. Ampère has done in a Memoir read to the Institute in January 1821, that these currents are established around the particles of magnets.⁶

Since the currents were now "established" around the particles, then the analogy collapsed, for again, as Ampère put it, "the magnetized needle in the interior of the hollow cylinder is always outside the currents, whereas in the helix, it is inside them."⁷ (See Fig. 3.)

Faraday's article on electromagnetic rotations was widely read. Its translation into French guaranteed that it would reach the whole European scientific community, for the *Annales de Chimie* was the most prestigious scientific journal of the day. Thus in the spring of 1822, Ampère was publicly committed to the denial of coaxial currents and to the existence of molecular currents in magnets. That fact is important when we come to assess his reaction to his discovery of induction.

Not everyone was happy with Ampère's explanations. On 5 July 1822, one of Ampère's Swiss friends, the Baron Maurice, wrote him a letter in which he quoted an anonymous, but clearly scientifically respectable, gentleman's

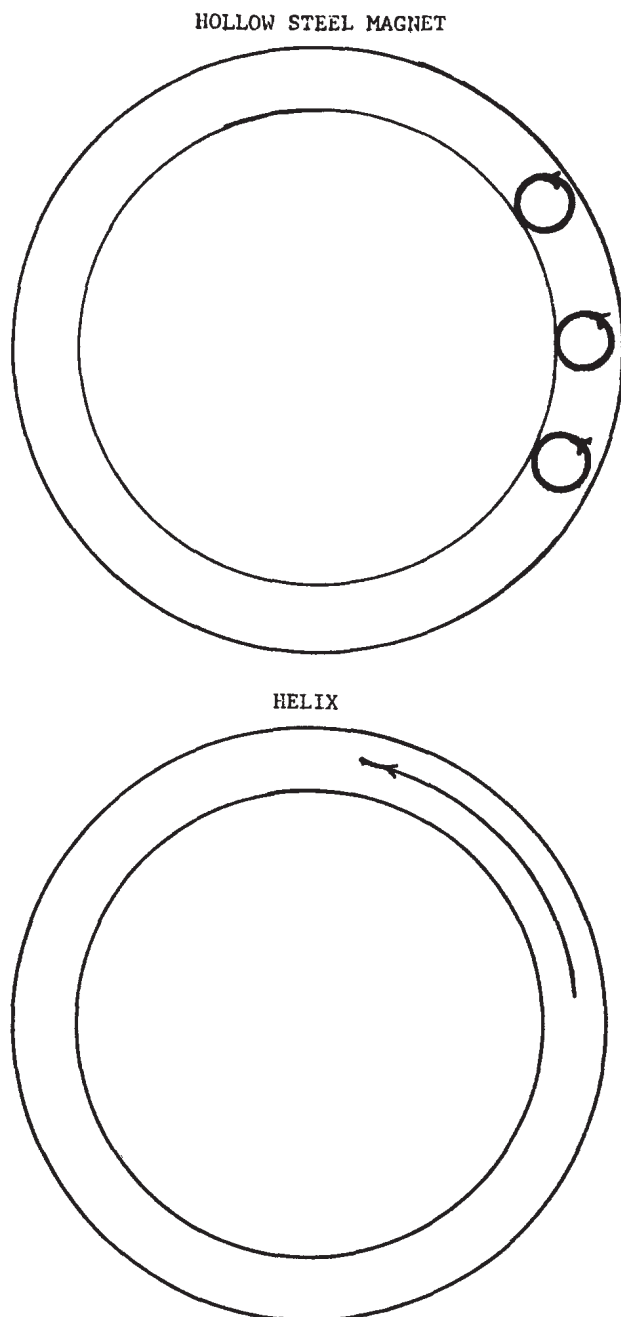


Fig. 3. The difference between Faraday's hollow steel magnet and Ampère's helix illustrated. The currents in the steel magnet, Ampère insisted, were closed curves so the floating needle was outside the curves. The needle clearly is inside the circular currents in the helix.

critical remarks on Ampère's theory. What bothered the critic was, as he put it, "not so much the act of imagination necessary to envision the action of these (molecular) currents, as it is the great number of purely gratuitous hypotheses."⁸ Among these hypotheses was, specifically, the "modifications to his hypothesis of the disposition of the currents in magnetic bars" which the critic thought illegitimate for it involved the "abuse of the consideration of the infinitely small with which one can do anything one wants."⁹

This criticism was serious and was taken seriously by Ampère. One must view it today against the background of the physics of the time. The corpuscular theory of light had just received its death blows from Ampère's friend, Fres-

nel, and died partially because its greatest defender, Jean-Baptiste Biot, sought to preserve it by means of *ad hoc* hypotheses that concerned the properties of light corpuscles which, like Ampère's molecular currents, were infinitely small. In short, Ampère was warned against switching hypotheses back and forth to deal with theoretical difficulties.

This warning came at a critical time for Ampère. Later in that same summer of 1822, he visited Geneva, where he repeated his experiment with the copper ribbon and insulated coil. His assistant was the son of an old friend, Auguste de la Rive, and the only change in the experiment was that a very strong horseshoe magnet was substituted for the bar magnet. It is not clear from either Ampère's description or that of Auguste de la Rive¹⁰ exactly how the experiment was carried out. Ampère does say that "there was no action between the strip and the magnet before the current passed through the spiral surrounding it."¹¹ This does imply that the magnet was in place before the current was turned on, and that the repulsions and attractions that were observed were clearly the result of induction. Yet neither Ampère nor de la Rive remark upon the momentary nature of the current in the strip, which is what most modern physicists would expect. Dr. Eric Mendoza of Hebrew University has recently repeated the experiment and found that, in fact, the effect is not momentary, but lasting.¹²

What Ampère and de la Rive had unwittingly constructed was an overdamped ballistic galvanometer so that the deflection of the strip was quite noticeable. Neither de la Rive nor Ampère knew quite what to make of it. De la Rive merely reported that

This important experiment thus shows that bodies that are not susceptible to permanent magnetization by means of electric currents, as are iron and steel, can at least acquire a kind of temporary magnetization while they are under this influence.¹³

No explicit statement here of the induction of an electric current by another. Ampère, on the other hand, was somewhat clearer.

[T]his experiment, consequently, leaves no doubt on the production of electric currents by influence, unless one suspects the presence of a small amount of iron in the copper (strip) of which the mobile circuit was formed.¹⁴

What seems perfectly obvious is that neither thought the experiment of great importance. Indeed, Ampère concluded his report with the astonishing statement, "This fact of the production of electric currents by influence, although interesting in itself, is however independent of the general theory of electrodynamic action."¹⁵ What can this possibly mean? We have seen Ampère appeal to this experiment as a crucial one in the development of his ideas on the physics of electrodynamics when the results of the experiment were favorable to his theoretical views. Moreover, the effect was one that had been anticipated and eagerly sought by investigators from Faraday to Oersted and should, therefore, have been trumpeted from the rooftops. Why did Ampère not take advantage of this to secure the lasting fame after which he so clearly thirsted? Scholars are divided on this point. Ross¹⁶ attributes it to de la Rive's youth and inexperience in describing the experiment in such a way as not to emphasize the induction of an electric current, and Amp-

ère's negligence in following up his discovery. Blondel¹⁷ simply dismisses the 1822 results by saying (without evidence) that Ampère did not modify his position on molecular currents. This is both correct and not understandable as a bare statement. Hofmann¹⁸ argues that Ampère's discovery of induction was so overshadowed by his "discovery" at the same time that the elements of currents in the same line repelled one another that Ampère simply ignored it.

My analysis above would indicate, and I shall offer some evidence to support it, that Blondel is essentially correct, although she has not spelled out the reasons behind Ampère's actions. As we have seen, the notion of molecular currents was fundamental to Ampère after Faraday's paper criticizing his theory appeared. He could not give it up, and so what he did was to minimize his discovery of electromagnetic induction. Indeed, his actions can best be described as sweeping it under a rug. He was present in Geneva when de la Rive read his account, and could then have modified it. He made changes in de la Rive's article when it appeared in the *Annales de Chimie*, but he did not change the account of induction. These facts, I think, offer us an important clue as to what Ampère thought his induction experiment meant. If coaxial currents are ruled out, as they had to be if Ampère were to preserve his theory, then there was only one other possibility, namely, that the currents produced by influence were molecular.

Ross cites evidence that Ampère did, in fact, rule out coaxial currents, although he interprets the evidence to mean that all induced currents were rejected.¹⁹ The passage bears quotation.

It should result from this experiment, considering the similar action (of attraction and repulsion) exercised by the two poles of the horseshoe magnet on the same part of the circuit, that the influence of the electric current should have developed another electric current in the strip, such as one observes in a metallic wire connecting the two poles of a voltaic pile; but M. Ampère has assured himself since that this is not so.²⁰

That Ampère interpreted his experiment to mean that currents were induced around the particles of the copper strip is shown by a letter he wrote to John Herschel in 1825. But, first, a bit of background is necessary.

One of the most startling discoveries of this period was Arago's description of what he called magnetization by rotation and which has since been named Arago's wheel. The phenomenon is simple and puzzling. If a copper disk is rotated below a magnetic needle to which it is not connected in any way, the needle will start to rotate. Similarly, if a magnet is rotated below the copper disk, the disk will start to turn.²¹

Among the many attempts to explain this phenomenon were those made in England by Babbage and Herschel,²² who were attracted by Ampère's electrodynamic molecule and used it in their explanation of Arago's wheel. Their paper was read to the Royal Society in June 1825. In October, Ampère sent a letter to Herschel in which he alluded to the work done by him and Babbage. One passage in it is crucial.

I should have liked to chat with you on the beautiful experiments that you have made with M. Babbage on the action that M. Arago has discovered between a magnet

and a metallic disk when one of them oscillates or turns around a vertical axis.

The manner in which these phenomena are produced by the action of a current or a magnet on the *little electrical currents* developed in the first case by the contact of the acid, and in the second by the influence of the magnet, in accordance with the experiment that I did in Geneva in 1822 and which was published at that time by M. August de la Rive, is a subject on which I always regret not having the time to write about.²³

In the paragraph immediately preceding this one, Ampère has described how an electric current, "forming a very small closed and plane circuit" acts in such a way that an assemblage of such currents will represent a permanent magnet. To be sure, Ampère does not call these currents molecular, but they are clearly of that order of magnitude and it is obvious that he is still thinking in the same terms as in 1821. What his 1822 experiment proved to him was that electrical currents *were* induced, but they were not what modern commentators think they were, namely currents coaxial to the strip. Hence Ampère's failure to try and determine the direction of the currents which has puzzled everyone. He simply did not care. We can now answer the question posed by the title of this article.

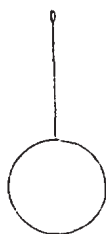
Ampère did not discover induction because it was the last thing he wanted to discover. To admit the coaxial currents that he had, in fact, produced would have been to place his precious theory in an impossible position. So he did what he had to do. He took the original experiment that he had devised to decide between coaxial and molecular currents and turned it into a meaningless exercise. The 1822 results showed that, no matter *what* he observed, he would insist upon interpreting it as proof of molecular, or at least, of molecular-sized currents. He had become a total prisoner of his theory.

Epilogue. Although our story is now complete as far as Ampère is concerned, there is still one aspect of this experiment that needs further treatment. Faraday heard about it almost immediately after it was performed in the summer of 1822. As he wrote to Auguste de la Rive's father, "M. Ampère's experiments which you mention are I think very important especially that of the production of magnetism in a piece of copper by mere vicinity to a voltaic circuit without actual connection with it..."²⁴ We do not have Gaspard de la Rive's letter to Faraday, so it is impossible to tell what his description of Ampère's experiment was like. Apparently, what Faraday got from it was that all that was necessary to induce an electric current was the propinquity of a current-carrying wire to another conductor. Faraday tried this in November 1825, but could detect no effect.²⁵ In 1828, he tried another variant, using a copper-wire ring into which a bar magnet was inserted, and then tested with other magnets, but again there was no effect. He did not report upon the actual experiment Ampère had done and described through de la Rive until after his discovery of electromagnetic induction in 1831. This I find impossible to believe. The experiment was simple enough to duplicate and I am convinced that Faraday tried it soon after he heard of it from Gaspard de la Rive. His *Diary* is very sketchy for the 1820s, so I think it probable that his record of it was not kept. What is interesting is that, when he does report upon it in his first series of *Experimental Researches in Electricity*²⁶ he gets it wrong. Instead of using a copper strip, he used a copper disk and, again as Dr. Mendoza has

Demonferrand



Cumming



(Actual Size)

Fig. 4. The illustrations available to Faraday could obviously be confused with copper disks, and Faraday seems to have so confused them.

shown in Ref. 12, the moment of inertia of the disk is too great for the effect Ampère observed to be visible. How could Faraday make such a mistake? The answer seems to lie in the vagueness of the descriptions of the experiment. De la Rive speaks of "une lame de cuivre" which could be read as a sheet of copper. Ampère's account, of course, was not published until after Faraday's death. A book on Ampère's ideas, published by his friend J. F. Demonferrand, was sent by Ampère to Faraday as an excellent summary of his theories. In that work Demonferrand speaks of "un cercle de cuivre" or a copper circle.²⁷ The English translation of this work by James Cumming repeats the phrase. Both works refer to a diagram, reproduced as Fig. 4. Demonferrand's looks a bit like a wire bent into a circle, although it could easily be confused with a disk. Cumming's is a disk. No wonder, then, that Faraday was misled.

We can now conclude with an interesting conjecture. What if Ampère had pushed his theory aside for a moment and reported fully and accurately on his 1822 Geneva experiment? Faraday certainly would have repeated it and, given his experimental genius, would have soon wrung from it the conditions necessary for the induction of one current by another, the directions of original and induced current, and all the other familiar conclusions that followed from his later, independent, discovery of electromagnetic induction in 1831. But Ampère would have had the glory of the original discovery and he would not have had to plead, rather pitifully, for his share in it, as he did in 1832. There is, I think, an important lesson to be learned here.

¹For the history of Ampère's early discoveries in electrodynamics, see L. P. Williams, *Isis* 74, 498 (1983).

²For the difficulties involved with helices, see Ref. 1, p. 498.

³M. Faraday, *Q. J. Sci.* 12, 74 (1821); reprinted in M. Faraday, *Experimental Researches in Electricity* (Taylor and Francis, London, 1839–1855), Vol. 2, pp. 127 ff.

⁴M. Faraday, Ref. 3, p. 144.

⁵*Ann. Chim. Phys.* 18, 370 (1821).

⁶Reference 5, p. 376.

⁷Reference 5, p. 376.

⁸L. de Launay, *Correspondance du Grand Ampère* (Gauthier-Villars, Paris, 1936–1943), Vol. 3, p. 924.

⁹Reference 8, p. 924.

¹⁰Ampère's description was given in a report he read to the Academy of Sciences on 16 September 1822 and not published until 1885 in J. Joubert, Ed., *Collection de Mémoires Relatifs à la Physique: Book II: Mémoires sur l'Electrodynamique* (Société, 1885), p. 333 ff. Auguste de la Rive devotes only a single paragraph to this experiment at the end of *Bibl. Univers.* 21, 29 (1822) and republished (with important changes) in *Ann. Chim.* 21, 24 (1822).

¹¹Ampère in Joubert, Ref. 10, p. 334.

¹²E. Mendoza, *J. Eur. Phys.*, to be published (1986). I am grateful to Dr. Mendoza for sending me a copy of the typescript of this paper, which is the best account of this experiment extant.

¹³A. de la Rive, Ref. 10, p. 48.

¹⁴Reference 11.

¹⁵Reference 11.

¹⁶S. Ross, *Notes Records R. Soc. London* 20, 184 (1965).

¹⁷C. Blondel, *Ampère et la Creation de l'Electrodynamique* (Bibliothèque Nationale, Paris, 1982), p. 119.

¹⁸J. Hoffman, "Electromagnetic Induction and Ampère's Evaluation of Experimental Evidence in Electrodynamics: 1820–1823," unpublished; submitted to *Osiris*.

¹⁹Reference 16, p. 196.

²⁰A. C. Becquerel, *Ann. Chim.* 25, 272 (1824); Ross¹⁶ translates "Il resulterait" as "One would have concluded," which may mislead the reader into thinking that Ampère had, in fact, so concluded: There is no evidence for this.

²¹For a good account of this discovery and its early history, see Ref. 16, p. 193 ff.

²²C. Babbage and J. F. W. Herschel, *Philos. Trans.* 467 (1825); for a detailed account of their experiments and conclusions, see L. P. Williams, *Michael Faraday, A Biography* (Chapman and Hall, London, 1965), p. 170 ff.

²³Ampère to Herschel, Paris, 25 October 1825; original in the Royal Society, Hs. 1. 394; published in D. Gardiner and R. Gardiner, *Bull. Soc. Amis André-Marie Ampère* 23, 13 (April 1966).

²⁴L. P. Williams, Ed., *Selected Correspondence of Michael Faraday* (Cambridge U.P., Cambridge, 1971), Vol. 1, p. 138.

²⁵T. Martin, Ed., *Faraday's Diary* (Bell, London, 1932), Vol. 1, p. 279.

²⁶M. Faraday, *Experimental Researches in Electricity*, Ref. 3, Vol. 1, paragraphs 2 and 78.

²⁷J. F. Demonferrand, *Manuel d'Electricite Dynamique* (Bachelier, Paris, 1823), p. 173.